

ROUNDTABLE: Kuhn and Lederberg on Scientific Thought

A Meeting of Biological And Philosophical Minds

IS scientific thinking different? The Princeton psychologist George A. Miller reports that when he proposed a scholarly talk on this question, he was reminded of the old Henny Youngman joke that begins, How's your wife? The irresistible answer in both cases is: Compared to what? But seriously, those who think about the subject are often struck by how little is known of "scientific thinking" — and they are sometimes alarmed by how loosely the term can be employed for distinctly unscientific ends. The *Week in Review* invited two men with a professional interest in the subject to think out loud: Dr. Thomas S. Kuhn, onetime physicist and now a philosophy professor at Massachusetts Institute of Technology, whose enormously influential book "The Structure of Scientific Revolutions" punctured the notion of scientific change as a strictly rational process, and Dr. Joshua Lederberg, a Nobel laureate in biology and president of Rockefeller University, whose concerns include communications between scientific disciplines that have their own research processes and languages. The topics they discussed with New York Times editors and reporters included science and politics, science and change, and unpopular theories. Excerpts follow.

Plain Sense, The 'Method' And Creation

Q We discuss science as if we understand what it is. The issue doesn't seem that simple. Those who disbelieve the theory of evolution, for example, have proposed in its stead creationism. Is creationism a science?

Dr. Kuhn. I don't have a nice set of necessary and sufficient conditions for being a science. But with creationism, there isn't a research basis. The activities and objectives involved are not those of solving a succession of internally generated puzzles. In those terms, I don't think creationism is a science.

Q. What made you try crossing bacteria?

Dr. Lederberg. Well, the work of other scientists had made it important to discover whether bacteria had genes or a genetics that was consistent with the mainstream of genetics research in mice and other animals. Before that time, the issue may not have been so important.

Q. Did you meet with disbelief?

Dr. Lederberg. I might have. But coincidentally, (in 1946) there was at Cold Spring Harbor the first postwar symposium on genetics of microbes. I don't think anyone important in the field was left out. People kept saying how exciting new work was, but 'Isn't it too bad bacteria don't have a sexual process.' It was an irresistible setting in which to say, 'But they do.' And I did.

There followed several hours of intense, critical discussion during which all the correct questions could be put and the bulk of scientists could convince themselves that my experiments had no

forms don't make a difference. But in most cases, they've made a difference because of particular things going on in the sciences, which were then permitted to come together or facilitated in doing so. If the fields had been in different states or if other fields had been put together, again it's likely nothing would have happened.

Dr. Lederberg. Permissions for disciplines to meet one another aren't that easy to come by. Many institutional settings would not allow scientists to change the character and direction of an investigation or to enter fields in which they did not have credentials. It would argue that creating environments where these things are permissible, even if you can't force two nuclei to fuse, is an important issue of science management.

Separating Politics From Politicization

Q To what extent do you think that science today has been politicized? There was, for example, the trial in Salt Lake City where scientists disputed whether radioactivity from atomic tests in the 1950's caused subsequent disease among Utah citizens. It seems that science asks people to believe in the reliability of results. Yet here was conflict.

Dr. Lederberg. Why do you call that politicization?

Q. Radioactivity is a politically charged issue.

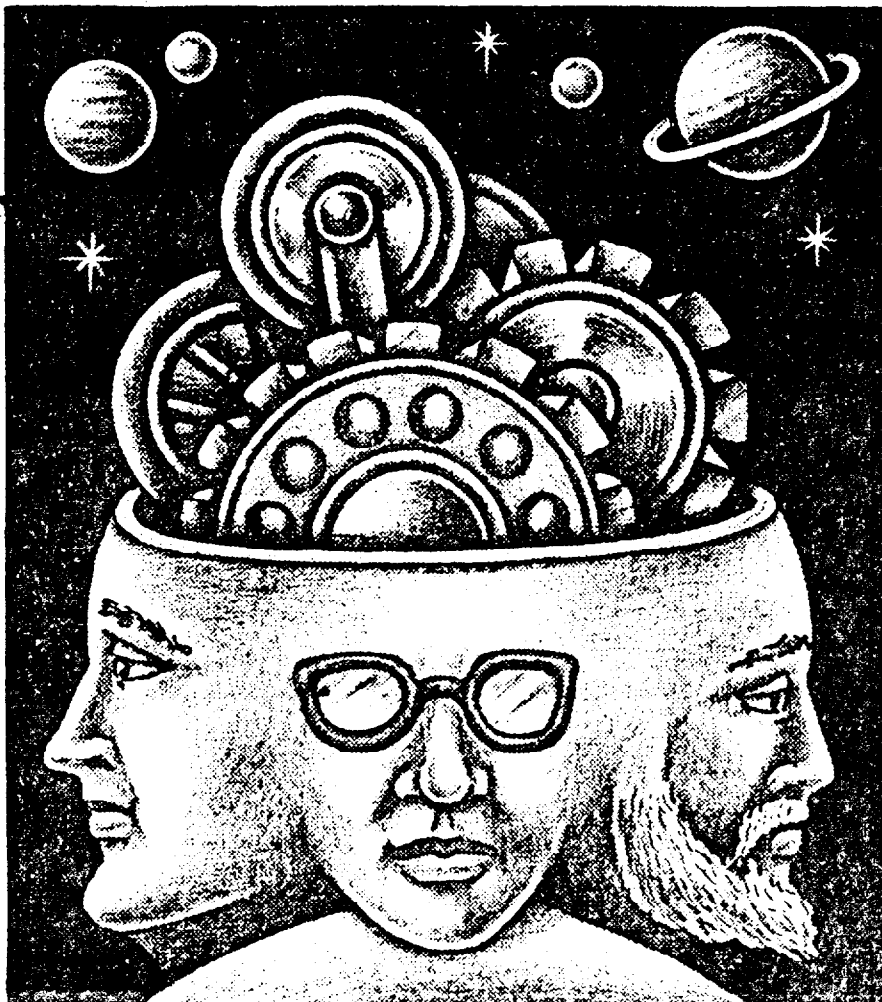
Dr. Kuhn. Radioactivity is something discovered and studied by scientists that has gotten involved deeply with questions that are political. But I would like to talk about that as something that involves scientists with politics, not as the politicization of science.

There are more areas today than before where the products of research are of vast social consequence. Thus, scientists are more involved in questions with political overtones. That makes problems. One of them is public misapprehension of the extent to which expertise, say in radioactivity, enables a scientist to provide the concrete information called for by policymakers.

Dr. Lederberg. In Utah, at least, a large part of the controversy concerns the amount of radioactivity released and the extent of human exposure to it. The answers have a lot to do with the records that were kept and the credibility of the institutions involved. Of course, people's institutional affiliations will have a lot to do with what they say about the authenticity and veracity of records of past events.

Q. Isn't that politics?

Dr. Lederberg. Yes, but it may not be politicizing science.



I would say there's a group here that makes knowledge claims. They don't make them on the basis of the same sort of enterprise scientists use. That would not mean that scientists are right and creationists are wrong, but it would make it absurd to suggest that the two enterprises are the same.

Q. Surely science is defined by its scientific method.

Dr. Kuhn. I don't believe there is something specifiable as a scientific method that is not pretty much what common sense calls for in most cases.

Dr. Lederberg. I would call creationism a theory of knowledge, because there is a pure form in which its propositions can be stated that is undefeatable. That form is that at some point in time the universe was created and that all the evidence we have access to, and all we will ever have access to, may mislead us into the belief that there was a long, prior evolutionary tradition. That is a logically undefeatable proposition, for having said it, what do you do next? You might posit that for various reasons, such as predicting what you'll discover when you next put a spade into the ground, you will further investigate the evidence to see what other consistent picture emerges. You then have the same enterprise sciences pursue!

Dr. Kuhn. In practice, however, creationists did not develop their viewpoint in that way and have not been tempted to do the sorts of things with their viewpoint that scientists have done.

Q. What about the notion that scientific method is largely common sense?

Dr. Lederberg. To the extent that very little by way of scientific demonstration follows formal procedures, it is pretty much common sense. Scientists use methods we should recognize in daily life, but that we may not push to the ultimate. For example, how often do we ask whether an adversary's position is logically defeatable?

Dr. Kuhn. You're right. Science is more systematic. But its logical structure is very much the logical structure of common sense. In that sense, you're not going to be able to distinguish science from non-science on the basis of the method used.

Dr. Lederberg. But there's a relentlessness in science rarely found in everyday affairs. Indeed if we did find it, we'd call the person who operated like this compulsive.

Nonrational Reasons To Clutch at Old Beliefs

Q. A theme of Dr. Kuhn's book is that to an extent, non-rational factors make scientists resist new ideas. Dr. Lederberg, do non-rational factors play a role in genetics?

Dr. Lederberg. In my view, these factors are most important in the choice of problems for study. For example, I was startled — and privileged — at age 21 to have made a surprising discovery that involved merging bacteriology and genetics. That was contrary to the wisdom of the time, which held that bacteria could not be crossed since they had no genetics.

I've been puzzling about that ever since, because I felt the discovery should have been made 20 years before I was born. One can hardly give a rational explanation for the fact that it had not even been looked for.

Ideas & TrendsContinued



Dr. Thomas S. Kuhn



Dr. Joshua Lederberg

Drawing by Charles Waller; The New York Times/Jim Wilson

loopholes. Such critical forums rarely happen.

Q. But until your discovery, was there an unspoken dictum that bacteria do not have a genetics?

Dr. Lederberg. Oh yes. Certainly among bacteriologists. It's embodied in the class name, Schizomycetes or 'fission fungi.' In the scale of nature, it was one of the distinctions by which bacteria were differentiated from more complex organisms. It had been a philosophy of the distribution of life, deeply ingrained. In fact, it had become almost a canon of faith that if you were a pure bacteriologist working with uncontaminated cultures, they didn't do anything interesting from a genetic point of view.

Dr. Kuhn. Let me amplify your point, which seems too little appreciated. In a sense, bacteriologists were taught to stop bacteria from changing. Almost by definition, that's what was meant by bacterial cultures being pure. So at an early stage, built into the scientist's notion of a pure bacterial culture was the notion that it doesn't change. And the first thing you did when you saw changes was to assume impurity. That type of assumption made it hard to discover that there are genetically borne changes in bacteria.

So, you see, it isn't just stubbornness that leads people to hold on to an outmoded belief. This is something built into scientific language and technique. You'd have to change your ideas of the appropriate techniques for purification to accept a discovery of the sort Dr. Lederberg made.

Dr. Lederberg. How can we use such insights into scientific change to promote more rapid scientific progress? I wonder what Dr. Kuhn would advise that might rationalize the process?

Dr. Kuhn. I'm not sure as to how much rationalization need be done. Clearly there are times when you're going to say somebody's going overboard; there are extremes one wants to avoid.

But look, you've said that one reason your field didn't change earlier was because nothing had rubbed the noses of bacteriologists and geneticists in each other's work. My guess is that if someone had brought those groups together earlier on without something substantive on which to focus and evidence that it was a good area to focus on, nothing would have happened.

I am inclined to say, then, that evolutionary patterns and internal developments are what most fruitfully bring two groups of this sort together, and I'm dubious as to whether one can speed and rationalize the process.

Q. Is scientific change largely accidental then?

Dr. Lederberg. Institutional forms have consequences; one can do something about those forms.

Dr. Kuhn. I don't mean to say that institutional

Q. There is an interesting conflict here, since policymakers must pass legislation...

Dr. Lederberg. Yes, they want one-armed bandits.

Q. ... and they turn to scientists for a basis for rulemaking. But scientists don't always have the experimental data to provide such a basis. How should they deal with the legislators' demands?

Dr. Lederberg. The scientists' job is to tell them the health risks; value judgments belong to a larger sphere. That's a naïve theory of separation, but it's something we ought to aspire to.

Dr. Kuhn. I am not sure I agree. There are policy decisions to which scientific findings are relevant, but for which these findings are not precise enough nor the theories developed enough to permit analyses of outcomes in any but the

vague terms. If scientists then respond to pressure for definite, factual answers, they mislead policymakers. But if policymakers insist that only precise, factual answers will do, they reject the only help scientists can sometimes give. Under those circumstances, I'm not sure the fact-value dichotomy is the appropriate ideal.

Pursuing Odd Theories And Extraterrestrials

Q. What new proposition or discovery would be the most earthshaking for science today? The discovery of life forms in outer space?

Dr. Kuhn. It depends on what the life turned out to be. There's a story in contemporary philosophy that illustrates what I mean. A spaceship from Earth goes to a place called Twin Earth, which is very much like Earth. There's even this liquid that lies around in lakes, evaporates and rains down again. On Twin Earth it's called water. But when the chemist from Earth analyzes the liquid it's not H₂O, it's XYZ.

The message supposedly sent to Earth at this point is 'On Twin Earth water is XYZ not H₂O.' But that's absurd. The wire should have read 'Back to the drawing boards — our chemistry is wrong. It doesn't have a ruling for something that behaves like water and isn't H₂O.' That kind of discovery would be revolutionary, for it's incompatible with the fundamentals of existing science.

Dr. Lederberg. I would think that just finding a Twin Earth, in the sense of a planet with an evolutionary pattern similar to ours, would be shattering. It would imply determinism of a series of events to which we impute a random character.

Dr. Kuhn. Whether one should pursue this kind of search depends partly on how much it would cost and partly on how rewarding it would be to know the answers. We are going to have to husband resources for that sort of enterprise.

Q. But this husbanding comes when scientists hold to popular theories in virtually every field.

Dr. Kuhn. Are you suggesting that because of a resource shortage quirky people will be prevented from pursuing less popular theories?

Q. Yes.

Dr. Kuhn. That happens, and it raises a difficult issue of priorities. If unpopular views are to be given an adequate hearing, we must build additional machinery for experiments. That could be done, but only the political process can decide whether the costs would be justified.